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ABSTRACT

Skeletal models of the regression of occupational status on schooling that correct for response variability and incorporate a variance component structure are presented. The models were derived from an analysis which used multiple measurements of educational attainment and occupational status for 518 male high school graduates and their brothers. The first section of the document compares the simple regressions of occupational status on schooling between brothers without correcting for response variability. Section 2 specifies a structural model with distinct regressions of occupational status on schooling for families, primary respondents, and brothers. Section 3 develops a measurement model for regressions of status on schooling and compares the corrected regressions of primary respondents and their brothers. Section 4 combines the measurement model with a family variance component structure. The remainder of the document discusses possible elaborations and extensions of this work. (KC)

Some Structural Equation Models of Sibling Resemblance in Educational Attainment and Occupational Status

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Abstract

Multiple measurements of educational attainment and occupational status for 518 male high school graduates and their brothers in the Wisconsin longitudinal study are used to develop and interpret skeletal models of the regression of occupational status on schooling that correct for response variability and incorporate a family variance component structure. Methodological complications follow from the facts that the sample consists of sibling pairs, that primary respondents, rather than families are the sampling units, and that primary respondents are in some cases informants about their brothers. While the analysis provides a methodological template for the specification of more complete models of stratification, we find that the regression of occupational status on educational attainment is relatively insensitive both to response variability and to the specification of a common family variance component.

It is a sad fact that in doing empirical work we must continuously search for the passage between the Scylla of biased inferences due to left-out and confounded influences and the Charybdis of overzealously purging our data of most of their identifying variance, being left largely with noise and error on our hands. In a sense, we run into a kind of uncertainty principle: The amount of information contained in any one specific data set is finite and, therefore, as we keep asking finer and finer questions, our answers become more and more uncertain.

[Zvi Griliches 1977:13]

1.0 INTRODUCTION

Effects of background on social and economic achievement are not well-specified by parental, familial, and contextual variables that usually appear in multivariate models of the stratification process. That is, explicit measures of social background do not fully reflect the common influences of the family of orientation upon schooling and adult achievement, so effects of schooling will be overestimated. For example, Hauser and Featherman (1976:116-118) have estimated that in 1973 a little more than half of the resemblance in educational attainment between American men and their oldest brothers could be explained by measured factors of social background: father's education, father's occupational status, number of siblings, broken family, farm origin, Southern birth, Spanish origin, and race.

Many social and psychological factors in achievement are poorly represented by measured background variables. Siblings have a partly overlapping genetic heritage, and excepting the possibility of temporal change within the family of orientation, siblings share a set of parents and other relatives (including one another) with whom they each interact in ways that only partly reflect the social and cultural divisions in the larger society. There are other parts of the social environment, too, which do not involve the functioning of families in a narrow sense, but whose nature and influence varies from family to family. For example, the neighborhood and community in which the family resides and the schools attended by the children are of this character.

Sociologists and economists have long recognized the importance of measuring the effects of schooling. Its influence on such measures of success as occupational status and earnings serves on the one hand as an indicator of the role of educational institutions in fostering (or hampering) social mobility and on the other hand as an indicator of the productivity of personal and public investments in schooling. At the same time, it is well known that social and economic success may depend directly upon personal characteristics and conditions of upbringing that also affect the length and quality of schooling. For these reasons, it is by no means obvious that an association of schooling with social economic success can be interpreted in causal terms, and many studies have attempted to determine the degree to which such causal inferences are warranted.

The effects of background, broadly conceived, on achievement can be taken into account by modeling the similarity of siblings. This has helped to motivate a number of studies of the stratification process that are based upon samples of siblings, rather than of the general population, perhaps most notably within the massive study by Jencks and his associates (1979). Jencks et al. (1979:168-69) estimated regressions of occupational status on years of schooling in several samples of American men, controlling in various ways for family background and ability. Their analysis separated effects of schooling before and after high school graduation. They found large biases in effects of elementary and secondary schooling. For example, among men with brothers in the 1962 Occupational Changes in a Generation (OCG) sample, the slope of occupational status on early schooling declined by 22 percent, from 2.541 to 1.980 points on the Duncan (1961) SEI when eight measured background factors were controlled; it declined by an additional 11 percent to 1.699 when an unmeasured family education factor was specified. At the same time, the biases appeared to be less in estimates of the effects of post-secondary schooling. For example, among brothers in the 1962 OCG study the effect of 4 years of college fell only from 29.7 SEI points to 27.5 points when measured background was controlled and to 25.1 points when a family factor was specified. Even smaller biases were observed in Olneck's 1973-74 survey of Kalamazoo brothers.

In his critical review Griliches (1979) has noted a potentially significant methodological twist in the use of sibling-based research designs (also, see Griliches 1977). In a regression, say, of occupational status on schooling, random response variability in schooling will lead to more (downward) bias in the within-family estimator than in a naive regression that ignores family effects. This occurs because response variability necessarily occurs within individual responses, so a given component of unreliable variance in schooling looms larger relative to within-family variance than to total variance. Thus, the biases attributable to omitted background variables and to response variability are probably opposite in effect, and it is necessary to correct both at the same time.

In the late 1960s, little was known about the sensitivity of estimated parameters of models of the stratification process to response variability. Bowles' suggestion (1972; also see Bowles and Nelson 1974, Bowles and Gintis 1976) that retrospective proxy reports of parents' status characteristics were especially prone to error stimulated several validation studies; these have been reviewed by Hauser, Tsai, and Sewell (1983). Contrary to Bowles' expectation, improved control of response variability has not led to massive downward revisions in estimates of the effects of schooling on occupational or economic success (Bishop 1974, Bielby, Hauser, and Featherman 1977, Bielby and Hauser 1977). Moreover, use of a sibling-based research design renders moot the question whether social background variables have been measured accurately. At the same time, Griliches' argument makes it all the more important to correct for response variability in within-family regressions of adult success on schooling. To our knowledge, there have been no systematic efforts of this kind.

In the Wisconsin longitudinal study, we have pieced together multiple measurements of nearly all of the variables in a fairly large model of the stratification process (Hauser, Tsai, and Sewell 1983) for about 2000 primary respondents (who graduated from high school in 1957) and for a randomly selected adult sibling. The present analysis uses multiple measurements of educational attainment and occupational status for 518 male high school graduates and their brothers to develop and interpret skeletal models of the regression of occupational status on schooling that correct for response variability and incorporate a family variance component structure. We term these models "skeletal" because they do not include explicit socioeconomic background variables, mental ability, or other social psychological variables. Methodological complications follow from the facts that the sample consists of sibling pairs, that primary respondents, rather than families are the sampling units, and that primary respondents served in some cases as informants about their brothers.

Despite the finding of Jencks et al. (1979), we expect to find omitted variable bias in estimated effects of post-secondary schooling in the Wisconsin sample. For example, among 2069 men with nonfarm origins who were working in 1964, the slope of occupational status declined from 8.65 SEI points per year of school to 7.45 points when mental ability and four socioeconomic background variables were controlled (Sewell and Hauser 1975:72,81). Among 1789 men for whom high school grades, significant other's influence, and aspirations had also been ascertained, controls for ability, background, and these additional variables reduced the slope from 8.50 points to 6.12 points (Sewell and Hauser 1975:93,98). Further, in a similar model estimated for 3,411 male respondents in the 1975 Wisconsin survey, Sewell, Hauser and Wolf (1980:571,581) estimated biases of 13.7 percent in the case of first full-time civilian occupation and of 32.9 percent in the case of current occupation.

The first section of the analysis compares the simple regressions of occupational status on schooling between brothers without correcting for response variability. There is reason to find differences between the regressions for primary respondents and their brothers because there is a floor on the schooling of primary respondents and because the brothers (but not the primary respondents) vary widely in age. To provide a baseline for comparison of estimates that have been corrected for response variability or for family effects, it is desirable to estimate one or more common or pooled regressions of occupational status on schooling. For example, we may want a baseline estimate of the regression among primary respondents, among their brothers, or among all siblings combined. Estimation of such pooled regressions is complicated by the facts that observations are paired across siblings and that there are multiple measurements of educational attainment and of occupational status for each sibling.

The second section of the paper specifies a structural model with distinct regressions of occupational status on schooling for families, primary respondents, and brothers. The sampling of brothers through respondents in the Wisconsin study leads to an interesting problem of identification. After proposing a solution to the identification problem, this section of the paper compares within- and between-family structural regressions based upon alternative measurements of educational attainment and occupational status.

The third section of the paper develops a measurement model for the regressions of status on schooling, and compares the corrected regressions of primary respondents and their brothers. The interesting issues here pertain to the fact that primary respondents served in some cases as informants about brothers and that in some cases the same survey items were used to obtain self-reports from primary respondents and their brothers.

The fourth section of the paper combines the measurement model with the family variance component structure. This section of the paper compares within- and between-family structural regressions, and it compares these with estimates that fail to compensate for response variability or for family effects. We close the paper with a discussion of possible elaborations and extensions of this work.

2.0. THE WISCONSIN SIBLING DATA

The Wisconsin data have been accumulated over the years from a random sample of more than 10,000 men and women who were seniors in the state's public, private, and parochial high schools in 1957; for a description and review of the study, see Sewell and Hauser (1980). In 1957 detailed information was collected on the social origins, the academic ability and performance, and the educational aspirations of the students. There were successful follow-up surveys of the total sample (with approximately 90 percent response rates) in 1964 and in 1975. The first follow-up, a mail survey of the parents of the primary respondents, yielded educational histories and reports of marital status, occupation, and military service. The 1975 telephone survey yielded additional first-hand reports of social background characteristics, educational and occupational experiences, marital and fertility histories, and formal and informal social participation.

Most important for the present purpose, the 1975 survey obtained a roster of the siblings of the primary respondent, including date of birth, sex, and educational attainment. For a randomly selected sibling, the survey ascertained current address and occupation. In 1977, telephone interviews were conducted with a sample of the selected siblings (aged 20 to 65) that had been stratified by the size of the sibship, the sex of the sibling and the primary respondent, and the birth order and educational attainment of the sibling. Of 879 brothers of male primary respondents who were selected into this supplement, telephone interviews were completed with 749 (85.2 percent). There is reason to believe that the achieved sample of brother pairs adequately reflects the composition of the sample of primary respondents (and their brothers) from which it was drawn (Sewell and Hauser 1982:7-13). For the present analysis, we further restricted the sample to those 518 pairs of brothers aged 20 to 50 for whom the nine variables listed in Table 1 had been ascertained. Only 19 pairs were lost because of the age restriction, but an additional 212 pairs lacked complete data. In many cases the missing data were due to school enrollment or absence from the labor force, rather than to item nonresponse.

As shown in Table 1, there are two indicators of the educational attainment of the primary respondent (EDEQYR, EDAT64) and of his brother (XEDEQYR, SSBED). The first member of each pair is a self-report and the second is a proxy report. In the case of the primary respondent, the proxy report (EDAT64) was coded from the educational history in the 1964 follow-up, and in that of the brother, the proxy report (SSBED) was given by the primary respondent in the 1975 survey. In both cases there is some slippage in time between the self and proxy reports, and consequently some true educational mobility may appear as response variability in later models. To minimize this problem, as well as that of classifying post-graduate education in years, we have followed the Census practice of collapsing schooling at or beyond 17 years.

All of the occupation reports have been coded using materials from the 1970 Census and transformed into the Duncan SEI metric (Duncan 1961, Hauser and Featherman 1977:Appendix B; detailed industry and class of

Table 1: Description of the variables, mnemonics, source of report, and year of measurement: Wisconsin brothers (N = 518)

Mnemonic	Description	Source	Year
1. EDEQYR	Respondent's Years of Schooling	Respondent	1975
2. EDAT64	Respondent's Years of Schooling	Parent	1964
3. XEDEQYR	Sib's Years of Schooling	Sibling	1977
4. SSBED	Sib's Years of Schooling	Respondent	1975
5. OCSXCR	Respondent's Current Occupation	Respondent	1975
6. OCSX70	Respondent's 1970 Occupation	Respondent	1975
7. XOCSXCR	Sib's Current Occupation	Sibling	1977
8. OCSSIB	Sib's 1975 Occupation	Respondent	1975
9. XOCSX70	Sib's 1970 Occupation	Sibling	1977

Note: Occupation is scaled on Duncan's Socio-Economic Index.

worker were used in some instances to refine the scale values reported by Hauser and Featherman for certain occupation lines.) There are self-reports of the primary respondent's occupational status in 1970 (OCSX70) and in 1975 (OCSXCR). There are self-reports of the brother's occupational status in 1970 (XOCSX70) and in 1977 (XOCSXCR), and there is a proxy report (by the primary respondent) of the brother's occupational status in 1975 (OCSSIB).

As in the case of educational attainment, there is some spread in the temporal referents of these measurements, and some true status mobility may appear to be response variability. There are two reasons for our decision to treat the indicators for each brother as measures of the same occupational status construct. First, even over a period of several years, unreliability looms large relative to mobility as a component of observed change in occupational status (Bielby, Hauser, and Featherman 1977). Second, our preference is not to depict the true status of the individual at an instant in time, but a relatively stable feature of his placement in the occupational hierarchy. Thus, our concept of response variability in occupational status is inclusive of true short-run changes in status.

Table 2 reports the means and standard deviations of the nine status variables and their intercorrelations. All of the following analyses are based upon these data. Note that brothers have slightly less schooling than primary respondents, but are more variable in schooling than respondents. There is a similar pattern in the case of occupational status. This reflects basic differences between the populations of primary respondents and of brothers that are represented in the Wisconsin sibling data. There is a floor on the schooling of

Table 2: Product-moment correlation coefficients, means, and standard deviations: Wisconsin brothers (N = 518)

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1. EDEQYR	1.000								
2. EDAT64	0.906	1.000							
3. XEDEQYR	0.404	0.437	1.000						
4. SSBED	0.419	0.450	0.926	1.000					
5. OCSXCR	0.552	0.525	0.251	0.252	1.000				
6. OCSX70	0.590	0.562	0.300	0.295	0.818	1.000			
7. XOCSXCR	0.217	0.243	0.622	0.568	0.264	0.315	1.000		
8. OCSSIB	0.217	0.245	0.627	0.593	0.265	0.307	0.815	1.000	
9. XOCSX70	0.228	0.257	0.628	0.575	0.247	0.275	0.819	0.780	1.000
Mean:	13.60	13.38	13.37	13.29	4.91	4.88	4.80	4.72	4.49
St.Dev:	2.09	1.83	2.27	2.22	2.44	2.41	2.57	2.51	2.54

Note: Correlations are based on 518 pairs of brothers for whom complete data were available. For explanation of mnemonics, see Table 1. For convenience in the scaling of coefficients, values of the Duncan SEI have been divided by 10.

primary respondents, but not of their brothers; none of the former obtained less than 12 years of regular schooling. Moreover, nearly all of the primary respondents were born in 1939, while the age of their brothers varied widely over the range from 20 to 50. These cohort and age differences between the primary respondents and their brothers may also have affected the joint distributions of educational attainment and occupational status.

In the construction of structural models of sibling resemblance, the usual procedure is to treat the members of a given sibling pair as unordered or indistinguishable (Jencks et al. 1972, 1979; Olneck and Bills 1980). Common family factors affect each member of the pair in the same way, and there is only one within-family regression. The analysis treats families, rather than persons, as units of analysis. For each variable and family, there are observations on each member of the fraternal pair, but it does not matter which observation is which. This greatly simplifies data analysis. For example, regardless of the pattern of common (family) causation, regressions of inter-pair differences yield unbiased estimates of within-family regressions. In the present research design, where brothers are sampled through a narrowly defined cohort of primary respondents, symmetry between brothers in the joint distributions of variables cannot be assumed, but must be demonstrated empirically.

3.0 SIMPLE REGRESSIONS OF STATUS ON SCHOOLING

Table 3 displays the 10 zero-order regressions of own occupational status on own schooling, 4 among primary respndents and 6 among their brothers. Considering the heterogeneity of populations, informants, and

Table 3: Least squares regressions of occupational status on educational attainment for primary respondents and their brothers: Wisconsin brothers (N = 518)

Dependent variable	Independent variable	Parameter	Estimate	Standard error
OCSXCR	EDEQYR	ψ_{51}	0.643	0.058
OCSXCR	EDAT64	ψ_{52}	0.701	0.066
OCSX70	EDEQYR	ψ_{61}	0.678	0.059
OCSX70	EDAT64	ψ_{62}	0.739	0.066
XOCSXCR	XEDEQYR	ψ_{73}	0.702	0.058
XOCSXCR	SSBED	ψ_{74}	0.656	0.058
OCSSIB	XEDEQYR	ψ_{83}	0.691	0.057
OCSSIB	SSBED	ψ_{84}	0.669	0.058
XOCSX70	XEDEQYR	ψ_{93}	0.701	0.058
XOCSX70	SSBED	ψ_{94}	0.657	0.058

Note: Duncan SEI scale values have been divided by a factor of 10. Standard errors are estimated to take account of the clustering of observations within families and persons. Parameters (ψ_{ij}) are labeled as in Figure 1.

temporal referents, these regressions are remarkably similar. The two extreme estimates — both of which pertain to the primary respondent — are 0.643 and 0.739, and the remaining estimates cluster in the range from 0.65 to 0.70.

To establish a baseline for later comparisons, we want to obtain pooled estimates of the zero-order regressions for primary respondents, for their brothers, and for all persons regardless of response status. We want to know whether these several estimates are significantly different from one another. Further, we want to learn the sources of differences, if any, among the estimates. These appear to be straightforward problems, but they are complicated by two facts: (1) that there are two measurements of educational attainment for each brother and (2) that the sibling's equation is not independent of the primary respondent's equation because of the family linkage.

The general form of the regression is

$$y_R = \beta_1 x_R + \varepsilon_R \tag{1}$$

$$y_S = \beta_2 x_S + \varepsilon_S \tag{2}$$

where y_R and y_S are measurements of socioeconomic status of respondent and sib, respectively, x_R and x_S are the respective measurements of educational attainment, and ε_R and ε_S are disturbances, $Cov(\varepsilon_R,\varepsilon_S) \neq 0$. Further, there may also be cross-sibling covariances, $Cov(x_R,\varepsilon_S) \neq 0$ and $Cov(x_S,\varepsilon_R) \neq 0$. The first complication is that we have multiple observations of x_R and x_S, but we want a single estimate of β_1 and a single estimate of β_2. The second complication is related to the fact that

$$\begin{aligned} Cov(y_R,y_S) = {} & \beta_1\beta_2\, Cov(x_R,x_S) + Cov(\varepsilon_R,\varepsilon_S) \\ & + \beta_1 Cov(x_R,\varepsilon_S) + \beta_2 Cov(x_S,\varepsilon_R), \end{aligned} \tag{3}$$

where $\mathrm{Cov}(x_R,x_S)$ is, in general, not equal to zero. The implication of $\mathrm{Cov}(y_R,y_S) \neq 0$ is thus that β_1 and β_2 have to be estimated jointly subject to $\mathrm{Cov}(\varepsilon_R,\varepsilon_S) \neq 0$, $\mathrm{Cov}(x_R,\varepsilon_S) \neq 0$ and $\mathrm{Cov}(x_S,\varepsilon_R) \neq 0$, which is reminiscent of seemingly unrelated regressions (Zellner 1962).

A simple rescaling model permits us to specify several hypotheses about β_1 and β_2 and, at the same time, allows multiple measures of x_R and x_S. Figure 1 is a symbolic representation of this model in the LISREL notation (Jöreskog and Sörbom 1978). However, some of the notation of the LISREL model has been omitted here and in later analyses when the full notation is redundant. For example, in Figure 1, each occupational status variable is a construct (η) in the LISREL model, and the (trivial) measurement model has been ignored. Similarly, we ignore the disturbances (ζ) of the constructs (η) where they are redundant, and denote $\mathrm{Cov}(\eta_i,\eta_j)$ by ψ_{ij}. For the convenience of the reader, here and in later models we use consistent numbering, as well as mnemonics, to refer to the observables.

Let y_R be measured by OCSXCR $= \eta_5$ and x_R by EDEQYR $= \lambda_1\eta_1$, so $\mathrm{Cov}(\eta_5,\eta_1) = \psi_{51}$. Then

$$(4) \qquad \hat{\beta}_1 = b_{\mathrm{OCSXCR,EDEQYR}} = \frac{\mathrm{Cov(EDEQYR,OCSXCR)}}{\mathrm{Var(EDEQYR)}}$$

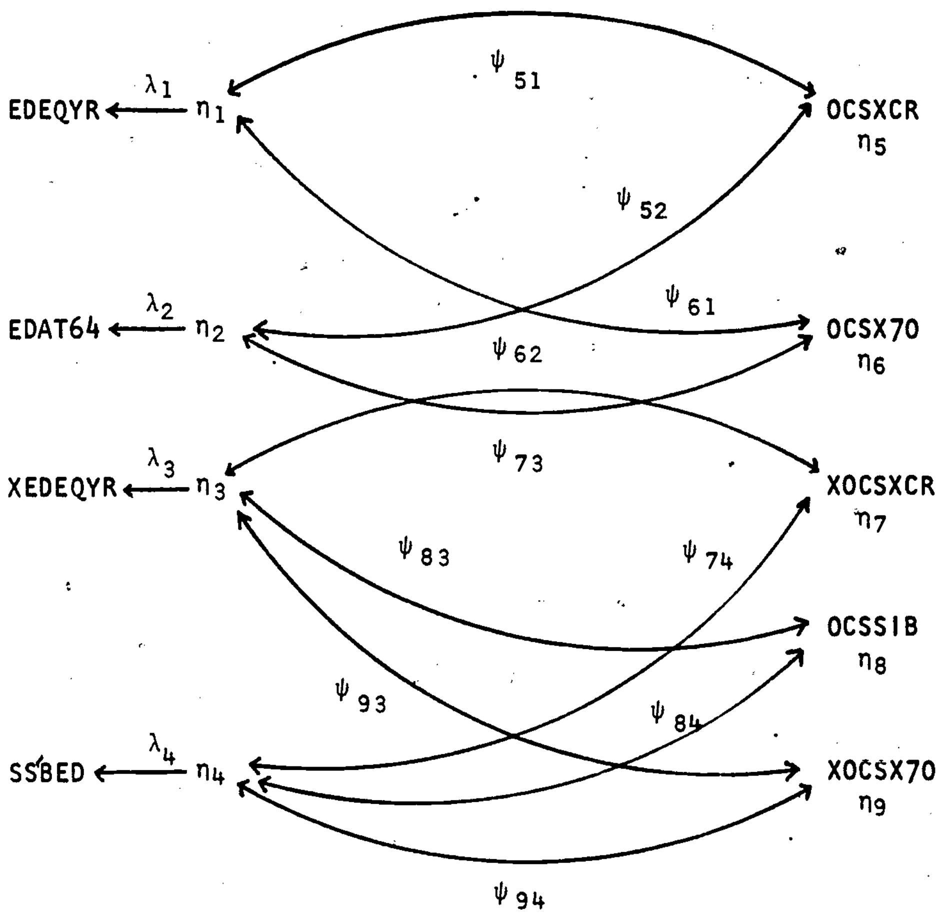
$$= \frac{\mathrm{Cov}(\lambda_1\eta_1,\eta_5)}{\mathrm{Var(EDEQYR)}} = \frac{\lambda_1\hat{\psi}_{51}}{\mathrm{Var(EDEQYR)}}$$

It follows that if we fix $\lambda_1 = \mathrm{Var(EDEQYR)}$ then $\hat{\beta}_1 = \hat{\psi}_{51}$. More generally, under the following choice of scalar transformations, namely,

$$(5) \qquad \lambda_1 = \mathrm{Var(EDEQYR)}$$

$$(6) \qquad \lambda_2 = \mathrm{Var(EDAT64)}$$

Figure 1. A LISREL model for testing homogeneity within and between siblings in regressions of occupational status on educational attainment



Note: The model includes all covariances among $\eta_1, \ldots, \eta_9$, but only those subject to constraints are shown and labeled. See text for explanation.

(7) $$\lambda_3 = \mathrm{Var(XEDEQYR)}$$

and

(8) $$\lambda_4 = \mathrm{Var(SSBED)}$$

the covariances (elements of Ψ) between the four education indicators and the 5 indicators of occupational status are rescaled as zero-order regression coefficients, and it becomes possible to impose the desired homogeneity restrictions directly in the LISREL model.

Table 4 shows goodness of fit and estimates of pooled slopes under four versions of the model of Figure 1. In Model 1, the four regressions pertaining to primary respondents have been pooled, yielding a slope estimate of 0.662. Under this specification, the heterogeneity is not statistically significant at the 0.05 level. In Model 2, the six regressions pertaining to brothers of primary respondents have been pooled, yielding a common slope estimate of 0.690; again, heterogeneity is not statistically significant. In Model 3, the two preceding sets of constraints are imposed at the same time; that is, there are distinct common slopes for primary respondents and for their brothers. Here, heterogeneity is of borderline statistical significance, and the common slope estimates are 0.666 for respondents and 0.679 for brothers. In Model 4, a single common slope is estimated to be 0.673, and the fit is negligibly worse than that of Model 3.

We conclude that there is very little evidence of heterogeneity in the zero-order regressions of occupational status on schooling between primary respondents and their brothers; indeed, there is more evidence of heterogeneity in the estimates for the same brother than between

Table 4: Constrained estimates of the regression of occupational status on educational attainment for primary respondents and their brothers: Wisconsin brothers (N=518)

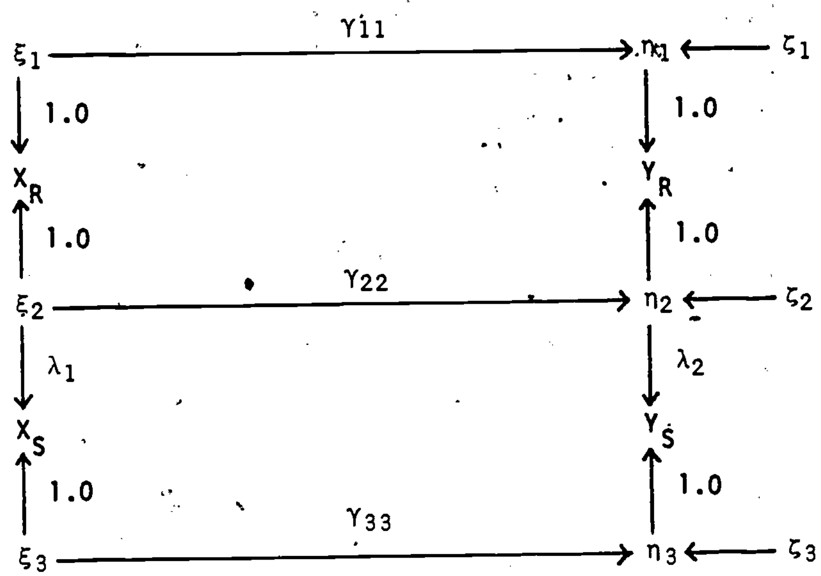
				Slope (std. error)	
Model of homogeneity	L^2	df	p	Respondent	Brother
1. Primary respondents	7.71	3	.052	.662 (.057)	-
2. Brothers	8.93	5	.112	-	.690 (.050)
3. Within siblings	16.73	8	.033	.666 (.057)	.679 (.054)
4. Complete	16.76	9	.053	.673 (.042)	.673 (.042)

brothers. We take the common slope estimates of Model 3 and Model 4 as the desired bases for comparison with estimates under models with response variability and/or a common family factor.

4.0 WITHIN- AND BETWEEN-FAMILY REGRESSIONS

Figure 2 gives the path diagram of a simple model of sibling resemblance in educational attainment and occupational status. (This specification of the family factors was suggested to us by William T. Bielby.) In the figure, the observations of educational attainment are denoted by X_R and X_S and the observations of occupational status are denoted by Y_R and Y_S for respondent and sibling, respectively. As shown in the central portion of the diagram, there are common family factors for educational attainment, ξ_2, and for occupational status, η_2, which are linked by the between-family regression, γ_{22}. The disturbances of the observables are the respective within-family components of educational attainment and occupational status for respondent and sibling. Thus, in the upper portion of the diagram, the within-family component of respondent's occupational status, η_1, is regressed upon the within-family component of his educational attainment, ξ_1; in the lower portion of the diagram, the within-family component of brother's occupational status, η_3, is regressed upon the within-family component of his educational attainment, ξ_3. The coefficients of the two within-family regressions are γ_{11} and γ_{33} for the primary respondent and his brother, respectively. In addition, the model includes scale factors, λ_1 and λ_2, that distinguish the effects of the two family factors on the educational attainments and occupational statuses, respectively, of respondent and sibling.

FIGURE 2. A structural equation model of sibling resemblance in educational attainment and occupational status



The path diagram in Figure 2 gives the appearance that any or all parameters of the model may differ between the primary respondent and his brother, but we cannot, in fact, make this assumption. The reason is that the model in Figure 2 is underidentified. As shown, the model has 11 parameters: 3 variances of ξs (ϕs), 3 error variances in ηs (ψs), 3 structural regressions (γs), and 2 scale factors (λ_1 and λ_2); but there are only 10 sample moments: 4 variances and 6 covariances among the 4 observable indicators. Thus, in order to estimate the model, it is necessary to impose at least one restriction on the parameters. We chose to impose the two restrictions $\lambda_1 = \lambda_2 = 1$; this implies that both pairs of within-family variables are in the same metric as the family factors and so justifies comparisons of slopes among the three regressions (Bielby 1982). This specification implies, also, that respondent-brother differences in variance are all due to within-family components of schooling and status; by construction, family effects on each sibling are the same.

We experimented with other identifying restrictions, for example, $\psi_{11} = \psi_{33}$, which says that disturbance variances are equal in the two within-family regressions. However, this restriction does not equate the metrics of the two within-family slopes. Fortunately, to anticipate our empirical findings, the data for respondent and sibling are so nearly symmetric that in retrospect the choice of initial identifying restrictions does not seem as serious a matter as we first thought it to be.

One plausible form of causation is excluded from the model of Figure 2, that is, the direct influence of one sibling upon the other. All "family" influences are carried by the common family factor. We shall comment briefly in the conclusion about modification of the model to incorporate unidirectional or mutual influence between siblings.

Table 5 shows goodness of fit and selected parameter estimates for several versions of the model of Figure 2. That model uses only one indicator of educational attainment and of occupational status for each member of the sibling pair, and we have selected three combinations of indicators for analysis. In the model of Panel A, we use the self-reports of educational attainment and occupational status at the survey date. A priori, we take these self-reports of current statuses to be of higher quality than the others, but the temporal referents of the occupations of respondent and sibling are separated by two calendar years. In the model of Panel B, we use the reports of the primary respondent about himself and about his brother in the 1975 survey. These pertain to the same calendar period, but the data about the brother are potentially suspect proxy reports by the primary respondent. In the model of Panel C, we use the self-reports of educational attainment and of occupational status in 1970. Here, the two occupation reports have the same temporal referent, but they are both also retrospective; further, the temporal ordering of occupation and educational attainment may be reversed.

With each combination of educational and occupational measures, we begin with a model that imposes equivalent scales on all of the variables, and we then test whether the parameters for respondents, brothers, and families are similar in other respects.

In Panel A the baseline model yields seemingly disparate slope estimates for primary respondents, brothers, and families. Indeed, the within-family slope estimate for primary respondents falls below the

Table 5. Maximum likelihood estimates of models of sibling resemblance in educational attainment and occupational status with latent family variables but no correction for response variability: Wisconsin brothers (N=518)

Variables and model	Slope (std. error) Respondent	Brother	Family	L^2	df	p
A. Self-reports of education and current occupation (EDEQYR, OCSXCR, XEDEQYR, XOCSXCR)						
1. $\lambda_1 = \lambda_2 = 1$	0.620 (0.074)	0.735 (0.05[illegible])	0.659 (0.074)	0.73	1	0.39
2. Add $\gamma_{11} = \gamma_{33}$	0.691 (0.047)	0.691 (0.047)	0.650 (0.074)	2.28	2	0.32
3. Add $\gamma_{11} = \gamma_{22} = \gamma_{33}$	0.676 (0.029)	0.676 (0.029)	0.676 (0.029)	2.44	3	0.49
4. Add $\psi_{11} = \psi_{33}$	0.676 (0.029)	0.676 (0.029)	0.676 (0.029)	2.52	4	0.64
5. Add $\phi_{11} = \phi_{33}$	0.676 (0.029)	0.676 (0.029)	0.676 (0.029)	6.65	5	0.25
B. Primary respondent's reports of education and 1975 occupation (EDEQYR, OCSXCR, SSBED, OCSSIB)						
1. $\lambda_1 = \lambda_2 = 1$	0.636 (0.074)	0.697 (0.062)	0.638 (0.073)	0.73	1	0.39
2. Add $\gamma_{11} = \gamma_{33}$	0.672 (0.048)	0.672 (0.048)	0.635 (0.072)	1.15	2	0.56
3. Add $\gamma_{11} = \gamma_{22} = \gamma_{33}$	0.658 (0.030)	0.658 (0.030)	0.658 (0.030)	1.28	3	0.73
4. Add $\psi_{11} = \psi_{33}$	0.658 (0.030)	0.658 (0.030)	0.658 (0.030)	1.32	4	0.86
5. Add $\phi_{11} = \phi_{33}$	0.658 (0.030)	0.658 (0.030)	0.658 (0.030)	3.51	5	0.62

Table 5, continued

Variables and model	Slope (std. error) Respondent	Brother	Family	L^2	df	p
C. Self-reports of education and 1970 occupation (EDEQYR,OCSX70,XEDEQYR,XOCSX70)						
1. $\lambda_1 = \lambda_2 = 1$	0.612 (0.071)	0.698 (0.058)	0.734 (0.071)	2.77	1	0.10
2. Add $\gamma_{11} = \gamma_{33}$	0.664 (0.046)	0.664 (0.046)	0.727 (0.071)	3.71	2	0.16
3. Add $\gamma_{11} = \gamma_{22} = \gamma_{33}$	0.687 (0.028)	0.687 (0.028)	0.687 (0.028)	4.08	3	0.25
4. Add $\psi_{11} = \psi_{33}$	0.687 (0.028)	0.687 (0.028)	0.687 (0.028)	4.26	4	0.37
5. Add $\phi_{11} = \phi_{33}$	0.687 (0.028)	0.687 (0.028)	0.687 (0.028)	8.39	5	0.13

range of estimates in the naive regressions (compare Table 3), while the estimate for brothers exceeds that for families. For primary respondents, the within-family estimate (0.620) is 0.96 times as large as the naive regression of OCSXCR on EDEQYR (0.643, reported in Table 3). At the same time, the within-family slope estimate for brothers (0.735) is 1.047 times as large as the naive regression (0.702). We shall see that this initial, equivocal finding on bias in the schooling-occupation relationship recurs throughout the analysis.

As shown in Line A2 of Table 5, there is no statistically significant difference between the within-family estimates for respondents and brothers. When this equality restriction is imposed, the fit deteriorates only by $L^2 = 1.55$ with 1 degree of freedom. The common, within-family slope estimate, 0.691, is nearly as large as the naive estimate for brothers, and it is actually larger than the common slope estimate based on the model of Figure 1 (0.673). Again, there is little evidence that the omission of common family variables significantly affects these estimates. In passing, it may be worth noting that the common, within-family slope estimate based on the model of Figure 2 is also larger than the estimate from the difference regression (0.663 with a standard error of 0.044).

In many areas of sociological analysis, it is often found that regressions across population aggregates -- like cities, regions, or organizations -- are steeper than corresponding individual regressions. This is (partly) the basis of the well-known literature on "ecological correlation" (Duncan, Cuzzort, and Duncan 1961) and "aggregation bias" (Hannan 1971). For example, the occurrence of heterogeneous within-school and between-school regressions of educational aspirations on socioeconomic status (Sewell and Armer 1966) was the source of a controversy that revolved around the question whether there were emergent "contextual" effects of schools or whether the individual-level regressions were misspecified (Hauser 1971, Boyd and Iversen 1979).

In the present case, then, we expected to find steeper between-family than within-family regressions of occupational status on schooling, but this proved not to be the case. In the model of Line A2, the within-family slope estimate is larger than the between-family slope. Moreover, as shown in Line A3 of Table 5, there is virtually no deterioration in the fit of the model when all three regressions are constrained to share a common slope. Sociologically, this is a remarkable finding, for it says that there is no emergent family effect on the relationship between educational attainment and occupational success; that relationship is just what we would expect from the differential rewards of schooling across individuals. Again, the common slope estimate is virtually the same as that estimated under the model of Figure 1.

In Lines A4 and A5 of Table 5, two more restrictions are added to the model; neither affects the slope estimates or their standard errors. First, we specify that $\psi_{11} = \psi_{33}$; this says that the disturbances in the two within-family regressions have the same variance. Under this additional restriction, there is virtually no change in fit. Second, we specify that $\phi_{11} = \phi_{33}$; this says that the within-family variances in educational attainment are the same for primary respondents and their brothers. Congruent with our expectations about selection into the sample, the data do not meet this restriction. The fit of the model deteriorates significantly ($L^2 = 4.13$ with 1 degree of freedom). Thus, with this one exception, the data of panel A do not depart significantly from the usual assumption of symmetry between siblings.

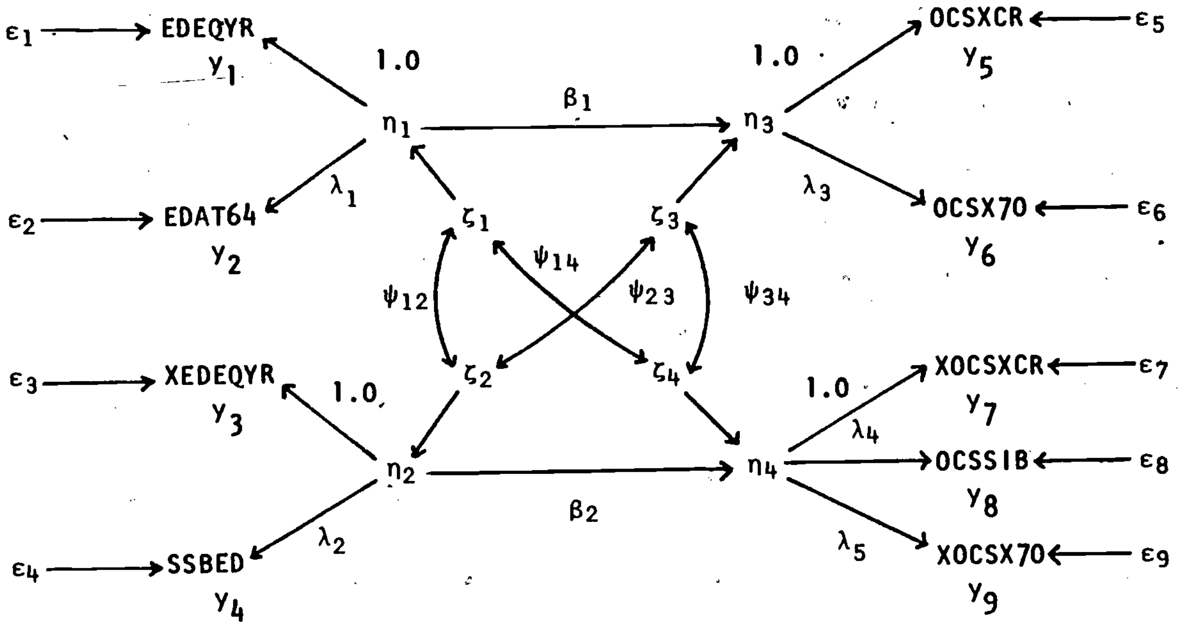
The findings in Panels B and C in Table 5 follow much the same pattern as those in Panel A, and we shall not review them in detail. One interesting difference is that in Panel B, where the data are all reports by the primary respondent, there is no significant difference between brothers in the variance of educational attainment. Throughout, the main finding is that of homogeneity in the regressions of occupational status on schooling, without regard either to the choice of indicators or to the specification of common family factors.

5.0 MEASUREMENT ERROR MODELS

Figure 3 displays a structural equation model that specifies distinct regressions of occupational status on schooling for primary respondents and their brothers and that incorporates response variability in each of the indicators of educational attainment and occupational status. In the structural portion of the model, there is a corrected regression for each brother, and the four cross-sibling covariances are not constrained by the model. The observables appear only as reflections or effects of the "true" educational and occupational constructs. The set-up is similar conceptually to that of Figure 1, but is simpler because there is only one "true" regressor per brother.

In this model we resolve the indeterminacy in the metrics of the latent variables (Bielby, Hauser, and Featherman 1977) by fixing the regressions of the self-reports of educational attainment on true

FIGURE 3. A model of distinct brothers' regressions of occupational status on educational attainment with errors in variables but no family factors



Note: See Table 6 for specification of error covariances.

education at 1.0 for respondents and siblings and by fixing the regressions of the self-reports of current occupational status on true status at 1.0 for respondents and siblings. This implies that the constructs are in the metrics of these indicators and that their variances are the true variances of the respective indicators. This is a convenient normalizing constraint because each of the reference indicators is a self report and because the same methods were used to ascertain and to code these variables for respondent and sibling.

The model of Figure 3 also includes selected covariances among response errors, which are not shown in the diagram. The initial specification of these error covariances is shown in Table 6. Covariances were permitted between the errors in any pair of variables that had been ascertained on the same occasion (or equivalently, from the same informant). Thus, the model permits all possible error covariances among reports by primary respondents and among reports by their brothers, but it permits no error covariances between reports by respondents and brothers, by respondents and parents, or by brothers and parents. One potential error covariance was not identified within the model, that between errors in the respondent's reports of his current occupation (OCSXCR) and his occupation in 1970 (OCSX70). We specified that error covariance to be equal to the corresponding error covariance for brothers, between XOCSXCR and XOCSX70, which is identified.

Table 7 shows measures of fit and estimates of the corrected regressions of occupational status on educational attainment in several versions of the model of Figure 3 and Table 6. The rows of Table 7 describe various restrictions on the measurement model. For each version of the measurement model, the left-hand panel (A) pertains to a

Table 6. Specification of non-zero error covariances in the model of Figure 3.

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1. EDEQYR	θ_{11}								
2. EDAT64	-	θ_{22}							
3. XEDEQYR	-	-	θ_{33}						
4. SSBED	$\overset{*}{\theta_{41}}$	-	-	θ_{44}					
5. OCSXCR	θ_{51}	-	-	$\overset{*}{\theta_{54}}$	θ_{55}				
6. OCSX70	θ_{61}	-	-	$\overset{*}{\theta_{64}}$	θ_{65}	θ_{66}			
7. XOCSXCR	-	-	θ_{73}	-	-	-	θ_{77}		
8. OCSSIB	$\overset{*}{\theta_{81}}$	-	-	θ_{84}	$\overset{*}{\theta_{85}}$	$\overset{*}{\theta_{86}}$	-	θ_{88}	
9. XOCSX70	-	-	θ_{93}	-	-	-	θ_{97}	-	θ_{99}

Note: θ_{65} is not separately identified and is estimated by $\theta_{65} = \theta_{97}$. Covariances marked "*" were not statistically significant in the baseline model of Table 7 and were dropped from all subsequent models.

Table 7. Selected models of the regression of brothers' occupational status on educational attainment with errors in variables: Wisconsin brothers (N=518)

Model	A. Distinct slopes					B. Common slope ($\beta_1 = \beta_2$)			
	β_1	β_2	L^2	df	p	β	L^2	df	p
1. Baseline model	0.672 (0.047)	0.708 (0.041)	10.51	9	0.31	0.693 (0.032)	10.88	10	0.37
2. $\theta_{41} = \theta_{81} = \theta_{54} = \theta_{64} = \theta_{85} = \theta_{86} = 0$	0.672 (0.047)	0.709 (0.040)	11.72	15	0.70	0.694 (0.032)	12.13	16	0.74
3. $\theta_{15} = \theta_{16} = \theta_{37} = \theta_{39}$	0.672 (0.047)	0.709 (0.040)	11.83	18	0.86	0.694 (0.032)	12.23	19	0.88
4. 3 plus all λs = 1	0.703 (0.044)	0.719 (0.036)	24.51	23	0.38	0.713 (0.029)	24.60	24	0.43
5. 3 plus $\lambda_2 = \lambda_3 = \lambda_4 = \lambda_5 = 1$	0.698 (0.043)	0.719 (0.036)	19.86	22	0.59	0.710 (0.029)	20.02	23	0.64
6. 5 plus $\theta_{11} = \theta_{33}$, $\theta_{55} = \theta_{77}$, $\theta_{66} = \theta_{99}$	0.687 (0.042)	0.720 (0.036)	29.58	25	0.24	0.706 (0.029)	29.96	26	0.27
7. 5 plus $\theta_{11} = \theta_{33}$, $\theta_{55} = \theta_{77}$	0.689 (0.042)	0.720 (0.036)	24.39	24	0.44	0.707 (0.029)	24.72	25	0.48
8. 7 plus $\psi_{33} = \psi_{44}$	0.689 (0.042)	0.720 (0.037)	24.44	25	0.49	0.707 (0.029)	24.77	26	0.53
9. 8 plus $\psi_{11} = \psi_{22}$	0.685 (0.039)	0.720 (0.038)	29.28	26	0.30	0.705 (0.029)	29.73	27	0.33

Note: Parameter restrictions refer to the model of Figure 3 and Table 6.

model with a distinct regression for each brother, while the right-hand panel (B) pertains to an otherwise similar model with a common slope for the two brothers. Under every measurement model in Table 7, the two regressions are homogeneous; the constrast between slopes yields test statistics on the order of 0.5 with 1 degree of freedom. Thus, our discussion focuses on comparisons among the rows of the table, which are virtually unaffected by the slope restriction, and for the most part we ignore the distinction between versions A and B of each model.

The baseline, Model 1, incorporates all of the error covariances in Table 6, and it specifies only normalizing restrictions on the slopes of indicators on constructs. That model fits well, but it is possible to specify a more parsimonious (and statistically more powerful) model by restricting selected parameters. In the baseline model, six error covariances were statistically insignificant, and these were dropped from Model 2: θ_{41}, θ_{54}, θ_{64}, θ_{81}, θ_{85}, and θ_{86}. Interestingly, these exhaust the possible terms pertaining to confounding between the primary respondents reports of his own and of his brother's status characteristics. The remaining, statistically significant error covariances all occur between reports about the same person. There is little difference in fit between Model 1 and Model 2.

The preceding models yielded similar estimates of the 4 covariances between errors in self-reports of educational attainment and of occupational status. In Model 3 these parameters were constrained to be equal with no significant deterioration in fit.

In Model 4 all of the slopes of observables on constructs were fixed at unity. This says that the true variance in every indicator of the same construct is equal, that is, every indicator of the same construct can serve equivalently to normalize the scale of the construct. In Model 4 the relationship between indicators and constructs conforms to the true-score model of psychometric theory. Fit deteriorates under this specification. For example, in the contrast between Model 4A and Model 3A, $L^2 = 12.68$ with 5 degrees of freedom, which is statistically significant with $p = 0.027$. We determined that the violation of these scale restrictions was due primarily to differences in the scales of the two reports of education of the primary respondent, EDEQYR and EDAT64. Thus, in Model 5 we relax the equivalence between the scales of these two indicators, while retaining the other 4 scale restrictions. There is a significant difference in fit between Model 5A and Model 4A ($L^2 = 4.65$ with 1 degree of freedom, $p = 0.031$), but none between Model 5A and Model 3A ($L^2 = 8.03$ with 4 degrees of freedom, $p = 0.091$).

To the restrictions of Model 5, Model 6 adds 3 equalities between error variances in pairs of indicators that were similar in content, that were self-reported, and that were ascertained and coded in the same way: EDEQYR and XEDEQYR, OCSXCR and XOCSXCR, and OCSX70 and XOCSX70. These restrictions do not all fit the data; for example, the contrast between Models 6A and 5A yields $L^2 = 9.72$ with 3 degrees of freedom, which is statistically significant with $p = 0.021$. By a forward selection procedure, we determined that this violation of the constraints in Model 6 was attributable to the estimation of common error variances based on reports of 1970 occupation, and in Model 7 we dropped this constraint. The fit of Model 7A is significantly better than that of Model 6A ($L^2 = 5.19$ with 1 degree of freedom), but it is not significantly worse than the fit of Model 5A ($L^2 = 4.53$ with 2 degrees of freedom).

Model 8 adds to Model 7 the restriction that the variances of the disturbances are the same in the regressions for the primary respondent and his brother. There is virtually no change in fit under this restriction. However, Model 9 adds the restriction that the variances in educational constructs are equal for primary respondents and brothers, and this leads to a statistically significant deterioration in fit. The contrast between Model 9A and Model 8A yields $L^2 = 4.84$ with 1 degree of freedom, $p = 0.028$.

It is instructive to compare the slope estimates in Models 7A and 7B with the common slope estimates in the naive regressions, reported in Table 4. The corrected estimate for primary respondents (0.689) is only 1.035 times larger than the common, uncorrected estimate (0.666); the corrected estimate for brothers is only 1.060 times larger than the common, uncorrected estimate (0.679). The corrected, common estimate for respondents and brothers (0.707) is only 1.051 times larger than the common, uncorrected estimate (0.673). These corrections ir slope are minimal because all of the indicators of educational attainment are highly reliable. Table 8 reports the reliabilities of the indicators and the correlations between response errors under the constrained measurement model. The reliabilities of the indicators of educational attainment range from 0.89 to 0.95; since slope corrections are inverse to the square root of reliability, the corrections are quite small.

The reliabilities of the indicators of occupational status are substantially lower than those of educational attainment, but unreliability in occupational status has no effect upon the slope estimates. Four of the five estimates are close to 0.75, and only the reliability of OCSSIB is as large as 0.84. The lower reliabilities of

Table 8. Reliabilities and error correlations in a measurement model of sibling resemblance in educational attainment and occupational status.

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1. EDEQYR	0.887	-	-	-	0.093	0.088	-	-	-
2. EDAT64	-	0.929	-	-	-	-	-	-	-
3. XEDEQYR	-	-	0.904	-	-	-	0.073	-	0.073
4. SSBED	-	-	-	0.948	-	-	-	-.044	-
5. OCSXCR	0.304	-	-	-	0.746	0.078	-	-	-
6. OCSX70	0.327	-	-	-	0.267	0.775	-	-	-
7. XOCSXCR	-	-	0.304	-	-	-	0.770	-	0.070
8. OCSSIB	-	-	-	-.284	-	-	-	0.835	-
9. XOCSX70	-	-	0.289	-	-	-	0.235	-	0.741

Note: Estimates are from model 8B in Table 7. Entries on the main diagonal are reliabilities. Entries below the main diagonal are correlations between errors in variables. Entries above the main diagonal are error covariances, expressed as proportions of the respective observed covariances. All of the error covariances are significantly different from zero at the 0.05 level.

the indicators of occupational status may reflect temporal spread as well as errors in reporting and processing the data. Of course, the unreliabilities in all of the indicators affect the estimated correlations between status variab. . The observed correlations between educational attainment and occupational status range from 0.525 to 0.590 for primary respondents and from 0.568 to 0.628 for brothers. In Model 8B the correlation between true educational attainment and true occupational status is 0.653 for primary respondents and 0.689 for brothers.

Correlated errors of measurement also affect the slopes and correlations between the educational and occupational constructs. The entries below the main diagonal of Table 8 are correlations between errors in the constrained measurement model. There are positive correlations of approximately 0.3 between errors in self-reports of educational attainment and of occupational status. These tend to compensate for random response variability by increasing the regressions (and correlations) between observed indicators of schooling and occupational status. At the same time, there is a negative correlation of about the same size between errors in the primary respondent's reports of his brother's educational attainment and occupational status, and this adds to the effect of random response variability by decreasing the observed correlation between those two variables. Last, there are positive correlations of approximately 0.25 between response errors in self-reports of occupational status; these positive, within-construct error correlations add to the effect of random response variability by decreasing the observed correlations between educational and occupational indicators.

As a practical matter, none of the correlated errors has a very large effect on slope estimates in the model. The error correlations are relatively large because the response error variances are relatively small. The entries above the main diagonal of Table 8 express the estimated error covariances as proportions of the respective observed covariances, and none of these is as large as 10 percent of an observed covariance.

6.0 A FAMILY FACTOR MODEL WITH RESPONSE ERROR

Figure 4 displays a structural equation model of sibling resemblance that combines the latent family structure of Figure 2 with the measurement model of Figure 3. While the path diagram in Figure 4 shows distinct, non-unit loadings for 5 of the observable variables, we report results only for models in which the measurement constraints of Model 7A in Table 7 have been imposed. Further, while the path diagram in Figure 4 shows distinct scale factors - γ_4 and β - for the effects of the family factors on the true educational attainment and occupational status of the brothers, we report results only for models in which these two coefficients have been fixed at unity in order to identify the model and normalize slope estimates.

Table 9 shows goodness of fit and slope estimates for several versions of the model in Figure 4. Because there are two scale restrictions in the family model, Model 1 incorporates one more restriction than Model 7A of Table 7, but the fit is not significantly affected. As in earlier models, the unrestricted slope estimate for primary respondents is less than that for families, which is in turn less than that for brothers. Model 2 adds the restriction of a common

FIGURE 4. A model of sibling resemblance in educational attainment and occupational status with errors in variables and latent family factors

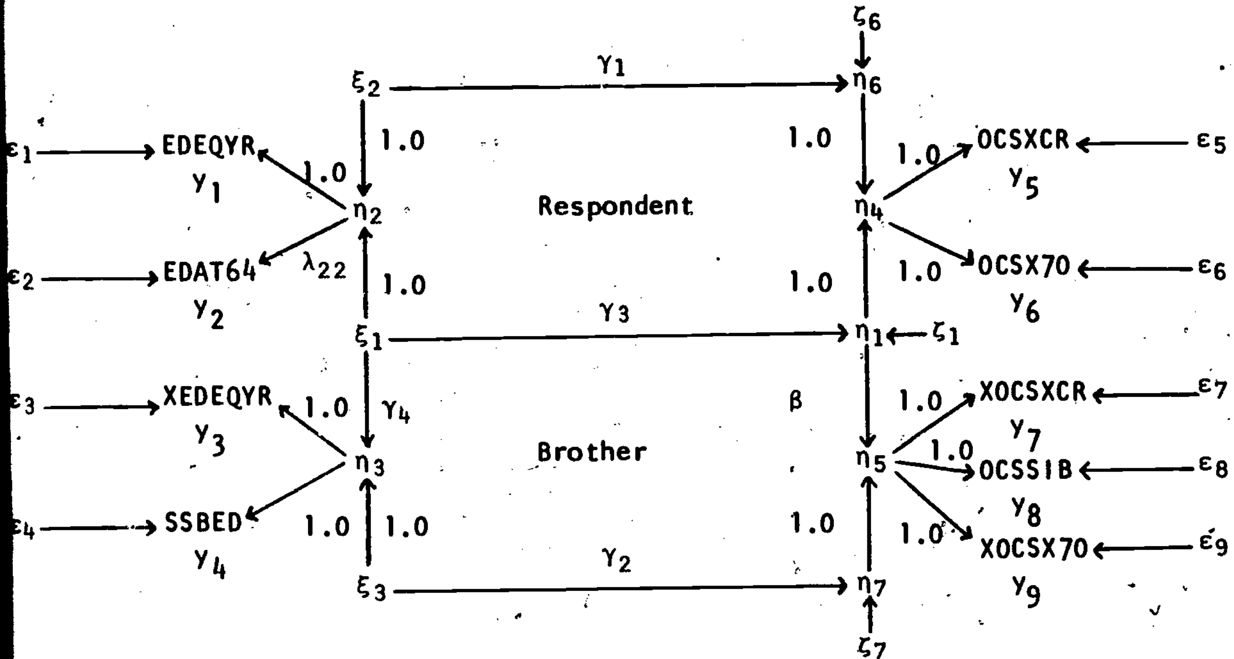


Table 9. Maximum likelihood estimates of models of sibling resemblance in educational attainment and occupational status with errors in variables and latent family factors: Wisconsin Brothers (N=518)

Model	Slope (std. error) Respondent	Brother	Family	L^2	df	p
1. $\gamma_4 = \beta = 1$	0.674 (0.081)	0.756 (0.057)	0.684 (0.062)	26.07	25	0.40
2. Add $\gamma_1 = \gamma_2$	0.728 (0.047)	0.728 (0.047)	0.678 (0.062)	26.74	26	0.42
3. Add $\gamma_1 = \gamma_2 = \gamma_3$	0.708 (0.029)	0.708 (0.029)	0.708 (0.029)	27.03	27	0.46
4. Add $\psi_6 = \psi_7$	0.708 (0.029)	0.708 (0.029)	0.708 (0.029)	27.07	28	0.51
5. Add $\phi_2 = \phi_3$	0.708 (0.029)	0.708 (0.029)	0.708 (0.029)	32.04	29	0.32

Note: These results are based upon the measurement model of Line 7A in Table 7.

slope for primary respondents and brothers, and this does not significantly affect the fit. As in some of the uncorrected models, the common, within-family slope estimate (0.728) is actually larger than the between-family slope estimate (0.678). Moreover, the common, within-family slope estimate under Model 2 is also larger than the common total slope estimate (0.707) in the constrained measurement model (Line 7B in Table 7).

Model 3 adds the restriction that all three slopes are homogeneous; again, there is no deterioration in fit. The common slope estimate, 0.708, is virtually the same here as in the measurement error model without the family factors, 0.707 (see Model 7B of Table 7). The common slope estimate in Model 3 is no more than 1.074 times as large as any of the uncorrected common slopes in the family models of Table 5 (0.676 in Panel A, 0.658 in Panel B, and 0.687 in Panel C); it is 1.051 times larger than the common slope estimate in the naive regressions (0.673 in Table 4). We are left with the strong impression that neither family factors nor response error have substantial effects upon our estimates of the occupational effects of schooling.

Model 4 of Table 9 adds the constraint that disturbance variances are the same in the two within-family regressions, and the fit is virtually unaffected by this. However, the data are not consistent with the addition of the restriction in Model 5 that true within-family variances in educational attainment are equal for primary respondents and their brothers ($L^2 = 4.97$ with 1 degree of freedom).

Model 4 of Table 9 is our preferred measurement and structural model, and Table 10 gives additional structural parameters of that model. If regressions of occupational status are homogeneous across

Table 10. Estimates of structural parameters in a model of sibling resemblance in educational attainment and occupational status with errors in variables and latent family factors: Wisconsin brothers (N = 518)

Parameter(s)	Estimate	Standard error
$\gamma_1 = \gamma_2 = \gamma_3$	0.708	0.029
$\psi_6 = \psi_7$	1.823	0.169
ψ_1	0.793	0.147
ϕ_1	1.991	0.217
ϕ_2	1.885	0.234
ϕ_3	2.730	0.256

persons and families, we hasten to add that this by no means denies the importance and visibility of families in the stratification process. For example, for primary respondents, 51.4 percent of the variance in schooling lies between families, and for their brothers 42.2 percent of the variance in schooling lies between families. Conditional on the hypothesis that true variance in schooling is the same for respondents as for their brothers — that is, on Model 5 of Table 9 — there is little difference between the within- and between-family variance components in schooling. The restriction that $\phi_{11} = \phi_{22} = \phi_{33}$ increases the test statistic only by $L^2 = 1.04$ with 1 degree of freedom relative to the restriction that $\phi_{22} = \phi_{33}$.

Of the total variance in occupational status — whether or not it is attributable to differences in schooling — 39.3 percent lies between families in the case of respondents, and 35.9 percent lies between families in the case of their brothers. For primary respondents and for their brothers, 30.3 percent of the variance in occupational status that is not explained by schooling lies between families. This last figure implies that the unexplained within-family and between-family variances in occupational status are by no means equal. For example, if we add the restriction $\psi_{11} = \psi_{66} = \psi_{77}$ to Model 4 of Table 9, the test statistic increases significantly by $L^2 = 15.98$ with 1 degree of freedom. Since the within- and between-family variances of schooling are not very different from one another, and the slopes are all homogeneous, the lower between-family variance in the disturbance of occupational status implies that the correlation between occupational status and schooling is larger between than within families. Under Model 4 of Table 9, the within-family correlations are 0.584 for primary respondents and 0.655 for their brothers; the between-family correlation is 0.746.

7.0 AN EXTENSION: CROSS-SIBLING EFFECTS

Siblings' achievements may be similar by virtue of modeling, tutoring, financing, or other directly facilitating roles and activities, as well as common upbringing. As early as 1911, Chapman and Abbott (604) wrote:

> the number of brothers among those who get on is large in comparison with the number of brothers which casual selection would pick out. The cause might be similarity of stock to some extent, but the most appreciable influence is probably the efforts and ambitions of parents and the help given to other members of his family by anybody who has made a step up in life.

Nonetheless, most models of sibling resemblance have specified common factor causation. Inter-sibling influences have been largely neglected. In other cases, age or ordinal position of siblings has been invoked to specify the direction of causality (from older to younger), thus sidestepping the issue of reciprocal influence (Baker and Johnson 1982). For example, Olneck (1976:198-214) showed that — with one minor exception — inter-sibling differences in achievement among Kalamazoo brothers were not related to age differences between them; he concluded that models of common family causation were at least as plausible as any of his own efforts to specify effects of older upon younger siblings. Where age differentials in sibling resemblance have been observed, these have alternately — and in statistically equivalent ways — been explained by variations in the strength of common factor causation

(Jencks et al. 1979:68-70) or in the influence of older upon younger siblings (Baker and Johnson 1982). Whatever the foundation of the assumption that older siblings affect younger siblings in research on childhood socialization or — more generally — in the methodological rule of thumb that temporal ordering implies causal ordering, we are doubtful about its wholesale application in studies of sibling resemblance that extend throughout the life-cycle. Successful younger, as well as older siblings may serve as role models, tutors, or social contacts. Even in the case of schooling, it is not clear that age is a valid indicator of temporal or causal precedence, and there is yet less reason to invoke it in the cases of occupational or economic standing.

It is well known that cross-sibling effects are not identified when families are the sampling units (so the data are symmetric), and there is a common factor for each pair of observations on siblings (Olneck 1976, Baker and Johnson 1982). In the structural portion of the model of Figure 4, this corresponds to the specification that $\gamma_4 = \beta = 1.0$, $\gamma_1 = \gamma_2$, $\psi_{11} = \psi_{33}$, and $\phi_{11} = \phi_{33}$, where β_{67} and/or β_{76} are the cross-sibling effects that we want to estimate.

Nonetheless, some of the models developed here may be modified to include direct unidirectional or reciprocal effects of the characteristics of one sibling on the other. We offer an illustrative, but by no means exhaustive list of these possibilities. In the model just described, either of the specifications $\gamma_1 = \gamma_2 = \gamma_3$ or $\psi_{11} = 0$ is sufficient to identify $\beta_{67} = \beta_{76}$. That is, either homogeneity of between- and within-family regressions or specification of a single family factor identifies the cross-sib effects. In the baseline structural model of Line 1 in Table 9, β_{67} or β_{76} or $\beta_{67} = \beta_{76}$ are identified without further restrictions. That is, one may postulate an effect of the primary respondent on the brother or vice versa, but not of each on the other, unless the reciprocal effects be equated. Since the baseline model postulates no equality restrictions between the two, within-family regressions, the equality restriction on cross-sibling effects appears unattractive in this case. Thus, it is difficult to imagine using one of these specifications unless one had an external criterion, like age, to determine the causal ordering of a cross-sibling effect.

In the model of Line 2 in Table 9, where the constraint of equal within-family regressions ($\gamma_1 = \gamma_2$) is added to the baseline model, equal cross-sibling effects ($\beta_{67} = \beta_{76}$) are also identified. Also, in the baseline model of Line 1 in Table 9, the specification that $\psi_{11} = 0$, that is, that there is a single family factor, identifies distinct cross-sibling effects ($\beta_{67} \neq \beta_{76}$). In fact, a good fit and plausible parameter estimates are obtained with the present data when both of these identifying restrictions are imposed simultaneously, that is, $\gamma_1 = \gamma_2$ and $\psi_{11} = 0$. Under this specification, the likelihood-ratio statistic of the model is $L^2 = 25.17$ with 26 degrees of freedom. The cross-sibling effects of occupational status are $\hat{\beta}_{67} = \hat{\beta}_{76} = 0.140$ with a standard error of 0.024, and the within-sibling regressions of occupational status on schooling are $\hat{\gamma}_1 = \hat{\gamma}_2 = 0.823$ with a standard error of 0.051. Thus, the within-family regressions are actually larger here than in the two-factor models without cross-sibling effects.

We have expressed some doubt about the use of age to specify the direction of cross-sibling influence. Using the multiple-group feature of the LISREL model, it is possible to investigate the effects of ordinal position and age-homogeneity on cross-sibling influences. That is, sibling pairs can be sorted into groups that are homogeneous in the size and/or direction of age differences between the members of the pair. Conditional on a model that identifies cross-sibling effects, one can test their homogeneity across groups.

8.0 DISCUSSION

We have intended this analysis to serve two purposes. First, we think that it yields significant findings about the influence of family background in the stratification process and about the importance of response variability in survey-based socioeconomic models. Second, we hope that it may serve as a methodological template for ourselves, and perhaps for others, in further research on the stratification process. We shall comment on each of these points in turn.

We have expressed a conventional model of sibling resemblance in the LISREL framework, thus facilitating the process of model specification, estimation, and testing. A useful innovation in this model has been our specification of distinct within- and between-family regressions. Conventionally the latter are not made explicit (Olneck 1976:139-149, 1977, Corcoran and Datcher 1981:195-197). We believe that the between-family slopes and, especially, their contrasts with the within-family slopes, are of real sociological importance. They show whether families enter the stratification system as relatively homogeneous, but neutral aggregates of persons, or whether they affect returns to the attributes and resources of their members (see Chamberlain and Griliches 1977:111). Further, we have incorporated random (and certain types of correlated) response errors in the model by obtaining multiple measurements of schooling and occupational status.

Within this framework, we have estimated regressions of occupational status on educational attainment among primary respondents and among their brothers, with and without response variability and common family factors. Paralleling Chamberlain and Griliches' (1975:428-432) analyses of schooling and income in the Gorseline data, we find little evidence that the omission of common family variables leads to bias in our estimates of the effect of schooling on occupational status. The between-family variance in schooling is about as large as the within-family variance, and there is substantial between-family variance in occupational status as well. Nonetheless, the regression of occupational status on schooling is homogeneous within and between families in the simple models we have estimated. This does not at all imply an absence of omitted-variable bias in the relationship between schooling and occupational status. As shown by Sewell and Hauser (1975) and Sewell, Hauser, and Wolf (1980), among others, the bias is substantial, but our finding suggests that intra-family differences in such variables as ability and motivation are its sources, rather than common family influences. The relationship between schooling and occupational success across families is just what we would expect from the differential rewards of schooling across individuals.

Moreover, this finding is insensitive to our treatment of measurement error. There is substantial unreliability (or at least, temporal instability) in occupational status, and there are small positive correlations between self-reports of one's own educational attainment and occupational status. At the same time, the reliability of educational attainment is extremely high. Even after purging the variance of schooling of its large between-family component, the regression of occupational status on schooling is not substantially affected by response variability in schooling. While this might be taken to encourage studies in which response variability in schooling is not or can not be specified, we think that application of our findings is not warranted and may lead to erroneous conclusions. Our findings pertain to a well-educated population in which years of schooling have been ascertained with a good deal of care and detail. Moreover, as additional (non-familial) explanatory variables — like mental ability — are added to the equations for educational attainment, the threat posed by response variability takes on greater importance.

Within the LISREL framework it has been easy to test a variety of hypotheses about symmetry between our primary respondents and their brothers in parameters of both the measurement and structural models. In extensions of the present work, we expect the similarity in measurement models to be extremely important; in some cases we have multiple measurements of a given construct only for primary respondents, and in other cases only for their siblings. Within the present framework, it is possible to "borrow" an estimate of error variance that is identified in one within-family segment of the model and use it in the other within-family segment of the model.

Another straightforward modification of the model permits us to test the factorial complexity of the latent family variates. For example, we can test the hypothesis that there is a single, unobserved family factor by setting $\psi_{11} = 0$; this yields an increase in the likelihood ratio test statistic of $L^2 = 31.30$ with 1 degree of freedom in the model of Line 1 in Table 9. Thus, we find that a single factor model is unacceptable (compare Hauser and Dickinson 1974, Jencks 1974); similar analytic issues will recur as we add explicit family background constructs to the model (Chamberlain and Griliches 1975, 1977).

The present model also lends itself to elaboration in a number of ways. First, it is possible to add more variables that have been observed (possibly with error) for respondent and sibling, and to specify their corresponding within- and between-family components and regressions. Perhaps the two most obvious constructs to be added in this fashion are mental ability and earnings, of which the former is an antecedent of schooling, and the latter is a consequence of schooling and occupational status. In the Wisconsin survey we have multiple observations of both of these variables among male primary respondents. Moreover, we have at least one observation for respondents and for siblings on each of the variables in the Wisconsin model of status attainment (Sewell and Hauser 1980, Hauser, Tsai and Sewell 1983). By using multiple indicators throughout the model, we shall be able to address such issues as "the endogeneity of schooling" with fewer trade-offs among specifications of errors in variables, simultaneity, and family effects (compare Griliches 1977, 1979).

Second, it is possible to add constructs to the model that are common to primary respondents and their siblings and that have no "within-family" components. Here the most obvious variables are shared characteristics of the family or community of orientation: parents' educations, occupations, and earnings; family size, ethnicity, and religious preference; community size and location. In most cases these variables will be specified as antecedent to other "between-family" variables. Again, these variables are subject to error, and in several instances we have multiple indicators of them in the Wisconsin data.

Third, beyond the specification of cross-sibling effects, there are other, and perhaps more interesting elaborations of the model that exploit the multiple group feature of LISREL. As noted earlier, the full Wisconsin sibling sample is based on a design that crosses sex by response status, so primary respondents of each sex are paired with randomly selected siblings of each sex. Thus, we can increase the statistical power of our analyses by fitting models within the multiple-group framework and pooling estimates where similar populations occur in different pairings, for example, male primary respondents paired with sisters as well as with brothers. More important, within this framework it will be possible to contrast parameters of the model between men and women.

While it is common to identify the family as a source of persistent social inequality, Griliches (1979:S60-S63) has offered several interesting speculations about ways in which the family may reduce inequality. These, too, lead to hypotheses about inter-group contrasts in parameters of sibling resemblance. For example, he suggests that families may try to invest their resources to minimize differences in outcomes between children, and he points to lower within-family than between-family regressions of schooling on IQ as possible evidence of this. Of course, the latter may also be artifacts of attenuation, since the previous argument about bias in the schooling coefficient applies equally well here. Another possibility is that family efforts to minimize differences in outcomes will be more successful as familial resources increase, say, as indicated by parents' socioeconomic status, and as sibship size decreases. Thus, we might look for reduced within-family regressions and within-family variance in smaller and higher-status families, relative to those in larger and/or lower status families. Given the observed secular changes in socioeconomic standing and in completed family size, changes in the family may be contributing to the reduction of social inequality.

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